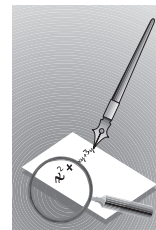


# commentary and analysis



- *The Differentiation between Grid Spacing and Resolution and Their Application to Numerical Modeling*
- *A New NASA Data Product: Tropospheric and Stratospheric Column Ozone in the Tropics Derived from TOMS Measurements*
- *Comments on "A Review of Cloud Seeding Experiments to Enhance Precipitation and Some New Prospects"*

## The Differentiation between Grid Spacing and Resolution and Their Application to Numerical Modeling

### Abstract

The purpose of this note is to suggest to the cloud and mesoscale modeling community that the two phrases *grid spacing* and *resolution* should not be used interchangeably.

### 1. Introduction

It is a common practice among those persons running numerical cloud and mesoscale models to use the two expressions *grid spacing* and *resolution* interchangeably. This practice may be a source of confusion to those reading published manuscripts on this subject.

### 2. Discussion

Manuscripts about numerical experiments generally contain a section describing the configuration of the domain in which the simulation occurred. This section is of the utmost importance because it provides the basic framework for interpretation of the model results. A fundamental piece of this information is the separation between two consecutive grid points, which is sometimes referred to as the *resolution* and sometimes as the *grid spacing*. An example can be found in, but not limited to, Wicker and Wilhelmson (1995). In their article are the phrases; "*fine-mesh simulation with 250-m horizontal resolution*" and "*the vertical resolution was rather coarse ( $\Delta z = 500$  m).*" The two

lengths, 250 m and 500 m, both refer to the horizontal and vertical *grid spacing*, respectively. Resolution refers to something different. It is impossible to resolve a wave with only two grid points (one  $\Delta x$ ), for example.

Even though Pielke (1991) discussed this issue, the practice continues. The following list of articles was extracted from the *Journal of the Atmospheric Sciences* from 1995 through 1998. Each of them used the two phrases interchangeably (Alexander et al. 1995; Rao and Agee 1996; Wu and Kurihara 1996; Orf et al. 1996; Trier et al. 1996; Yang and Houze 1996; Grabowski et al. 1996; Lee and Wilhelmson 1997; Lewellen and Lewellen 1997; Afanashev and Peltier 1998; Wang et al. 1998; Avissar et al. 1998; and Yuh-Lang et al. 1998).

In the earth's atmosphere, wave interactions produce an energy cascade to both larger and smaller scales. Energy at the smaller scales is removed by molecular dissipation. In a numerical model small-scale energy is erroneously aliased to large scales. These large-scale waves interact with other waves and generate an energy cascade in both directions. The resulting small-scale waves, again, alias energy to the large scales. This accumulation of energy at large scales leads to nonlinear instability and renders a numerical solution meaningless (Phillips 1959; Pielke 1984, p. 324).

A common method to minimize this process is to add a dissipative scheme that removes, for example,  $2\Delta x$  and  $3\Delta x$  waves. This means that the smallest resolvable wave is at least  $4\Delta x$  since  $2\Delta x$  and  $3\Delta x$  waves no longer exist in the numerical solution. Phillips (1959) was the first to point out the necessity of removing the small-scale waves. In the above example the horizontal resolution was at least 1000 m.

To be complete, the two terms *grid spacing* and *resolution* are also not interchangeable in linear models since small-scale wave speeds exhibit the largest errors when compared to the actual values (Pielke 1984, p. 279).

### 3. Conclusions

In conclusion, the two terms *grid spacing* and *resolution* refer to two different length scales that characterize a grid configuration. Because of this, they are not interchangeable in linear and nonlinear numerical models. It is not possible to resolve a wave on the scale of one grid spacing in any spacial direction. Since  $2\Delta x$  and  $3\Delta x$  waves are removed to prevent nonlinear instability, waves on the scale of at least  $4\Delta x$  may be resolved. Other terms that may be used in place of grid spacing are grid interval, grid length, and grid increment.

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## A New NASA Data Product: Tropospheric and Stratospheric Column Ozone in the Tropics Derived from TOMS Measurements

### Abstract

Tropospheric column ozone and stratospheric column ozone gridded data in the Tropics for 1979–present are now available from NASA Goddard Space Flight Center via either direct ftp, World Wide Web,

or electronic mail. This paper provides a brief overview of the method used to derive the dataset including validation and adjustments.

### 1. Introduction

Until recently the primary method to derive tropospheric column ozone (TCO) and stratospheric column ozone (SCO) from satellite data was by combining Total Ozone Mapping Spectrometer (TOMS) and Stratospheric Aerosols and Gas Experiment (SAGE)

ozone measurements (Fishman et al. 1990). TCO was determined by subtracting SAGE stratospheric column ozone from TOMS total column ozone. By the nature of the solar occultation method used for SAGE, measurements of SCO have limited spatial and temporal coverage. On average for any given month there are around only one to two measurement days within a given  $5^{\circ}$ – $10^{\circ}$  tropical latitude band. The limited coverage with SAGE prompts new approaches for deriving column ozone (Ziemke et al. 1998; Hudson and Thompson 1998; Thompson and Hudson 1999).

The objective of this publication is to provide information for the general community on obtaining a recently derived database of tropical TCO and SCO from NASA Goddard Space Flight Center.

## 2. Description of the data

Monthly averaged TCO and SCO data are derived in the Tropics for January 1979–present using the convective cloud differential (CCD) method of Ziemke et al. (1998). In the CCD method total column ozone is derived from low reflectivity ( $R < 0.2$ ) measurements and SCO follows from nearby column ozone measurements taken above the tops of very high tropopause-level clouds under conditions of high reflectivity ( $R > 0.9$ ). First, above-cloud column amounts are calculated in the Pacific region where tropopause-level clouds are persistent. SCO is then derived for every  $5^{\circ}$  latitude band and averaged from  $120^{\circ}\text{E}$  eastward to  $120^{\circ}\text{W}$  using only lowest values of above-cloud column amounts (the lowest values coincide with tropopause-level cloud tops). These SCO values are then assumed to be independent of longitude in a given latitude band. The assumption is based on the characteristics of zonal symmetry of tropical SCO as inferred from *Upper Atmosphere Research Satellite* (UARS) microwave limb sounder (MLS) and halogen occultation experiment (HALOE) ozone data. We refer the reader to Ziemke et al. (1998) for further details regarding the CCD method.

Horizontal resolution of the data is  $5^{\circ} \times 5^{\circ}$  with latitude range  $15^{\circ}\text{S}$ – $15^{\circ}\text{N}$  (centered on  $12.5^{\circ}\text{S}$ ,  $7.5^{\circ}\text{S}$ , . . . ,  $12.5^{\circ}\text{N}$ ). This latitude range was chosen based on observed zonal homogeneity of SCO in the tropical lower latitudes. Missing CCD data include some latitude bands for a few months and a large gap of 38 consecutive months (May 1993–June 1996) of missing data in which there were no suitable TOMS measurements available to apply the CCD technique. Although

*Meteor-3* TOMS from late August 1991 to December 1994 helps bridge the temporal gap between *Nimbus-7* and Earth Probe (EP) TOMS time periods, the *Meteor-3* TOMS data were not included in the CCD database. This is because of a non-sun-synchronous orbit of the *Meteor-3* satellite where even tropical measurements exhibit high solar zenith angles that greatly affect results from the CCD method.

## 3. Validation and adjustments

Validation of the CCD method is discussed by Ziemke et al. (1998) and further by Ziemke and Chandra (1999). These studies compared CCD data with ozonesonde and satellite measurements from UARS and HALOE.

The CCD TCO data include a new aerosol adjustment (Torres and Bhartia 1999) applied to the clear-sky low-reflectivity ( $R < 0.2$ ) TOMS total column ozone measurements. Because absorbing aerosols in the troposphere absorb backscattered UV, the amount of column ozone measured by TOMS in the presence of aerosols is underestimated. The aerosol adjustment makes a correction for this error. Globally, the largest adjustments [ $\sim 8$ – $10$  Dobson units (DU)] are over North Africa from around April through September and are associated with desert dust particles.

Figure 1 shows a 1979–99 climatology of the adjustment in the low-latitude Tropics for the months of January, April, July, and October. The largest adjustments occur in July (and also August, not shown) in the south Atlantic with values up to 6–8 DU. The adjustments in the Atlantic are caused by smoke from Africa and Brazil. The large values north of  $15^{\circ}\text{N}$  seen over Africa in July are caused primarily by desert dust coming from the Sahel and Sahara regions (spanning latitudes around  $10^{\circ}\text{N}$ – $28^{\circ}\text{N}$ ). The Sahel is a large dry grassland area south of the Sahara and has been especially dry since 1968. The aerosol adjustment also indirectly corrects for sea glint errors ( $\sim 1$ – $3$  DU) as can be seen in Fig. 1 over ocean in January south of  $10^{\circ}\text{S}$  and in July north of  $10^{\circ}\text{N}$ . The sea glint effect is a function of solar declination and is caused by bright surface reflection. The CCD TCO data include full month-by-month aerosol adjustment at all grid points.

In the study by Ziemke and Chandra (1999) it was noted that there appeared to be an instrument offset between *Nimbus-7* (N7) and EP TOMS CCD TCO measurements. In that study a constant 5 DU was subtracted from EP TOMS CCD TCO data relative to N7.

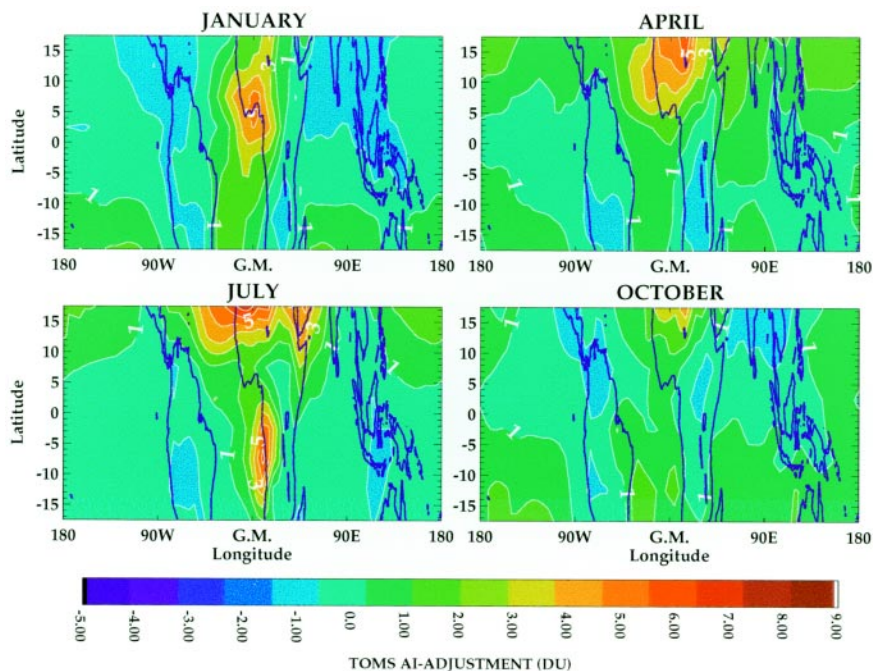


FIG. 1. Climatology (1979–99) of the aerosol adjustment (in Dobson units) applied to low-latitude TOMS clear-sky ( $R < 0.2$ ) total column ozone measurements for Jan, Apr, Jul, and Oct.

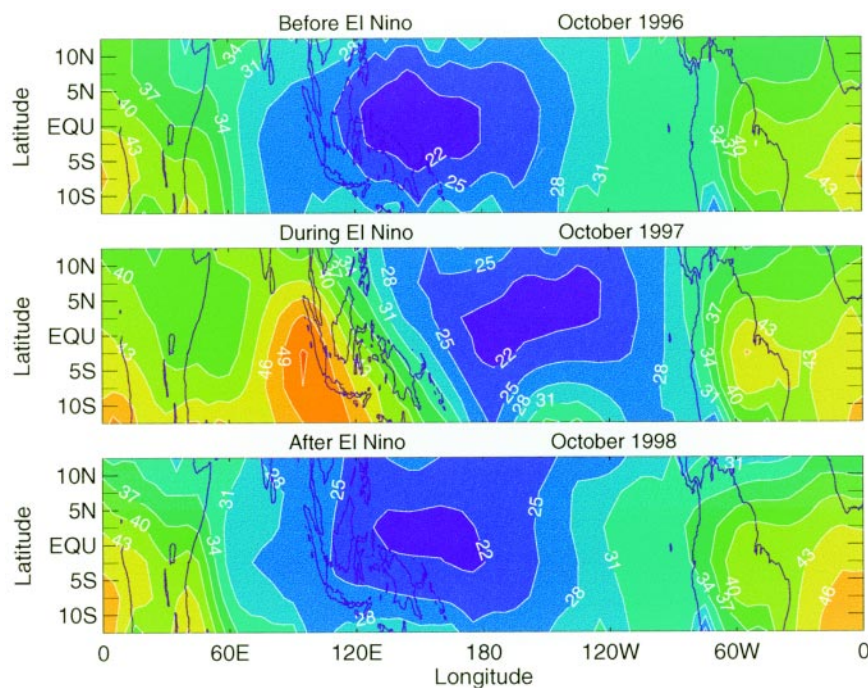


FIG. 2. Tropospheric column ozone (in Dobson units) in the Tropics derived from the CCD data that include month-by-month aerosol adjustment. (top) October 1996; (middle) October 1997; (bottom) October 1998. A constant 5 DU amount was subtracted from all EP TOMS TCO measurements to partially correct for potential instrument offset (see text).

This was partly based on direct comparison with ozonesonde TCO measurements from several tropical stations. Subtraction of the constant 5 DU amount did not affect analysis of the variabilities present in the data. A unique property of the CCD method for deriving tropospheric column ozone is that it is the only current approach not affected by interinstrument calibration errors (the CCD method differences only individual-instrument TOMS measurements of total column and SCO). The reason for the offset between N7 and EP instruments is not clear. Preliminary analysis indicates that it is related to a wavelength-dependent error in EP TOMS measurements that largely affects lower-reflectivity scenes. The archived data have no adjustments (such as the 5 DU constant amount noted). Users of these data must be aware that there may be an offset of several DU between N7 and EP TOMS TCO measurements. The CCD data contain valuable information regarding tropical TCO and SCO variabilities from monthly to decadal timescales. The study by Chandra et al. (1999) identified a solar-cycle signal [ $\sim 2$ – $3$  DU peak to peak] in TCO using N7 tropical CCD data. Another study by Chandra et al. (1998) identified an El Niño signal ( $\sim 5$ – $10$  DU) in TCO that appeared coupled to dynamical effects involving the shift in convection from the western to the eastern Pacific during El Niño. Figure 2 shows an example of this pattern shift by comparing October TCO data for the years 1996, 1997, and 1998. Low values of TCO east of the date line in October 1997 reflect the eastward

shift in convection during El Niño. The high values over Indonesia in October 1997 are related to biomass burning and the suppressed convection and change in dynamical transport in the region. The induced dry conditions over Indonesia during El Niño produced a large amount of uncontrolled wildfires. Recovery of the El Niño is seen for October 1998. The CCD data have also been used for studying seasonal and interannual variabilities in tropical tropospheric ozone to delineate the relative importance of biomass burning and large-scale transport (Ziemke and Chandra 1999).

#### 4. Obtaining the data

The CCD, TCO, and SCO data may be obtained via the World Wide Web ([http://hyperion.gsfc.nasa.gov/Data\\_services/Data.html](http://hyperion.gsfc.nasa.gov/Data_services/Data.html)) or direct ftp over the Internet: `ftp jwocky.gsfc.nasa.gov`; `login: anonymous`; `password: (your e-mail address)`; `cd pub/ccd`.

Because these are small datasets, the data can also be obtained via electronic mail from `ziemke@jwocky.gsfc.nasa.gov`

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#### Comments on “A Review of Cloud Seeding Experiments to Enhance Precipitation and Some New Prospects”

Bruintjes (1999, hereafter B99) has done a commendable job of providing a summary of recent developments in cloud seeding, the scope of present day commercial seeding programs, and pointing out some pitfalls that need to be avoided. This writer shares his cautious optimism about the future of cloud seeding, particularly due to the developments in hygroscopic

seeding that he describes, with his caveat, “if we do not oversell.”

However, some commentary beyond that supplied by B99 is required regarding two sets of cloud seeding experiments he describes. These are the experiments that took place in Israel, conducted by scientists at the Hebrew University of Jerusalem (HUJ) and those conducted at Climax, Colorado, by scientists at Colorado State University (CSU).

About the experiments in Israel, B99 states that “the original thought that clouds in Israel were continental in nature and that ice particle concentrations in



these clouds were generally small for cloud tops warmer than  $-12^{\circ}\text{C}$  with neither coalescence nor an ice multiplication process operating has also been questioned.”

The cloud-top temperature above which the HJ researchers reported low concentrations of ice crystals was  $-21^{\circ}\text{C}$ , not  $-12^{\circ}\text{C}$  as stated by B99. In fact, HJ researchers claimed that *no* detectable ice formed in clouds with tops warmer than  $-14^{\circ}\text{C}$  (e.g., Gagin and Neumann 1974; Gagin 1975, 1986). These claims gave wide credibility to the HJ experimenters’ statistical results suggesting that seeding had increased rainfall in the cloud-top temperature range of  $-12^{\circ}$  to  $-21^{\circ}\text{C}$  because few natural ice crystals, they claimed, formed in such clouds (e.g., Gagin and Neumann 1981). Also, the lack of seeding effects below cloud top temperatures of  $-21^{\circ}\text{C}$  was because ice crystal concentrations in those clouds averaged 10 or more per liter (e.g., Gagin and Neumann 1981).

While B99 notes that the HJ cloud reports have been questioned by Rangno and Hobbs (1995) and Levin (1992), he states that these measurements are “limited.” On the contrary, Rangno (1988) used 10 seasons of rawinsonde data to infer that there were problems with the HJ cloud reports: rain often fell from clouds with much higher top temperatures than could be accounted for by their reports. Further, the HJ reports that ice concentrations of one per liter did not appear, on average, until cloud-top temperatures had reached  $-17^{\circ}\text{C}$ , have been found to deviate substantially from a summary of ice-forming behavior in continental convective clouds around the world (Rangno and Hobbs 1988, 1995).

Further, Levin et al. (1996) provided additional information on the flight data gathered by Levin 1992, a study mentioned by B99. In only five days of sampling (six measurements) scattered over two winter months, Levin et al. (1996) found maximum ice particle concentrations of 60, 50, 300, 100, 20, and 50 per liter in clouds with tops “near”  $-10^{\circ}$ ,  $-6.5^{\circ}$ ,  $-13^{\circ}$ ,  $-10^{\circ}$ ,  $-11^{\circ}$ , and  $-10^{\circ}\text{C}$ , respectively. Recall that the HJ researchers asserted over many years that ice did not form at all in clouds with these top temperatures. All of these concentrations except one are also higher than the maximum concentration reported in a cloud for *any* cloud-top temperature by the HJ researchers.

While the Levin et al. (1996) sample can be considered small in the absolute sense, the implications are nevertheless mighty. The situation is analogous to a resort owner who has told tourists in his many brochures over the years that it has never rained at his resort in the winter. A tourist goes to this resort on five

different days one winter and it rains on every day. How confident can we be that the resort owner made exaggerated claims about the good weather at his resort? The answer is obvious (cf. Brier and Panofsky 1965). When B99 inadvertently discounts such results as “limited,” he misses their profundity.

Last, B99 makes no mention of perhaps the most astounding cloud seeding results yet in Israel, those from a recently reported third randomized experiment that consumed 18 years of seeding in central and southern Israel beginning in 1975. In this experiment, the seeded days averaged about 9% *less* rainfall than the control days (Rosenfeld 1998)! Cloud tops are, on average, lower and warmer in this region than in northern Israel (Gagin and Neumann 1974; Rangno and Hobbs 1995) making such negative results even more unexpected. It is probably fair to say that all of the disparate results have more than “somewhat” eroded the confidence in the HJ cloud seeding experiments.

B99 also describes some conclusions about the CLIMAX (sic) experiments based on Rangno and Hobbs (1987, 1993). B99’s description, however, was incomplete. While the *combined* result of the Climax I and II experiments was, as B99 reports, about 10% (Rangno and Hobbs 1987), our main conclusion was that Climax II had not replicated Climax I, a fact not mentioned by B99. This same important conclusion had been reached earlier by Rhea (1983). Replication, in particular, *independent replication*, is essential for the credibility of experimental results.

In fact, even the Climax I result, which contained the only statistically significant seeding results of the two experiments, is suspect. The reason for this is that when the results of Climax I are examined after the date the controls were selected by the experimenters, about midway through that experiment, no further seeding effects were observed (Grant and Mielke 1967; Rangno and Hobbs 1993). This inevitably raises the issue of whether “post-selection bias” (Dennis 1980)—“cherry-picking”—crept into the choices of control stations to show a seeding effect that the experimenters were sure was there. If the seeding effect is, in fact, illusory, and only the product of an extensive search, then it is extremely unlikely that it will be seen after the date the controls are chosen. This problem is analogous to a researcher finding historical climate “cycles” after a long search which then fail to appear in future data. This is what was observed in Climax I; a very large apparent seeding effect followed by no effect.

It should be pointed out, however, that the results of Rangno and Hobbs (1987, 1993) for the Climax experi-

ments are based solely on the Department of Commerce, National Oceanic and Atmospheric Administration (NOAA) cooperative recording gauge at Climax 2 NW. The data for this independently maintained recording gauge was reduced and published by employees of the National Climatic Data Center in Asheville, North Carolina, who of course had no knowledge of the random seeding decisions in the Climax experiments.

On the other hand, the researchers who conducted the Climax experiments have continued to claim, however, that a real seeding effect did fall on their own snowboards and gauges in these experiments (Mielke 1995)—while somehow avoiding the NOAA gauge located near the center of the target. This assertion by the experimenters, which cannot be independently verified, stresses the critical importance of having an independent collection *and archiving* of key data during cloud seeding experiments.

Further, the CSU researchers' claims of seeding effects in the Climax experiments are not backed by any viable evidence that the high 500-mb temperature stratifications in which they partitioned their seeding effects are related to any cloud microstructure property having great seeding potential. In fact, *unsuitable* conditions for ground seeding have been reported for the very stratification (high 500-mb temperatures) that they used on several occasions (e.g., Rangno 1979; Hobbs and Rangno 1979; Mielke 1979; Cooper and Marwitz 1980; Cooper and Saunders 1980; Rangno and Hobbs 1993). This was not mentioned by B99.

The same now appears to be true concerning the cloud microstructural foundation of the experiments in Israel.

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